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Is the mind Bayesian? The case for agnosticism

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Abstract This paper aims to make explicit the methodological conditions that should be satisfied for the Bayesian model to be used as a normative model of human probability judgment. After noticing the lack of a clear definition of Bayesianism in the psychological literature and the lack of justification for using it, a classic definition of subjective Bayesianism is recalled, based on the following three criteria: An epistemic criterion, a static coherence criterion and a dynamic coherence criterion. Then it is shown that the adoption of this framework has two kinds of implications. The first one regards the methodology of the experimental study of probability judgment. The Bayesian framework creates pragmatic constraints on the methodology that are linked to the interpretation of, and the belief in, the information presented, or referred to, by an experimenter in order for it to be the basis of a probability judgment by individual participants. It is shown that these constraints have not been satisfied in the past, and the question of whether they can be satisfied in principle is raised and answered negatively. The second kind of implications consists of two limitations in the scope of the Bayesian model. They regard (i) the background of revision (the Bayesian model considers only revising situations but not updating situations), and (ii) the notorious case of the null priors. In both cases Lewis' rule is an appropriate alternative to Bayes' rule, but its use faces the same operational difficulties.

Keywords Probability judgment • Subjective Bayesianism • Bayesian coherence • Probability revising • Probability updating • Linguistic pragmatics

1. Introduction

In the imposing literature devoted to the psychological study of probability judgment, the use of a theoretical model as a referential norm for "rational" behavior is a usual methodology.[1] Since the pioneering study by Rouanet (1961), and following the famous article by Edwards et al. (1963), the Bayesian model of probability has been the most frequently used as a normative reference or as a possible descriptive model. The implicit question that these studies attempt to answer is whether human beings perform probability judgment in a "Bayesian" manner. These interrogations go far beyond the realm of psychology: They also apply to the various domains that use the Bayesian model, such as economics (Davis and Holt 1993), law (Callen 1982), medicine (Casscells et al. 1978), artificial intelligence (Cohen 1985) and philosophy (Stich 1990).

The present paper is not directly aimed towards these important debates concerning probabilistic functioning in humans, but is rather a methodological examination of the *conditions* under which the Bayesian model may be used as a normative theory. In other words, it aims to find an answer to the following question: Does the psychological literature take into consideration the various implications and constraints imposed by the *usage* of the Bayesian model as a normative reference? It will be argued that the answer is negative, and the obstacles which the experimental methodology should overcome if the model were to be used will be reviewed. This paper will be organized as follows. In the next Section, the descriptive and normative uses of the Bayesian model made in the past will be reminded, and the main criticisms against using the Bayesian model reviewed. Then, the status of Bayesian model in the psychological literature and in probability theory will be examined in turn (Sections 3 and 4); these preliminary considerations are necessary to arrive at a definition of Bayesianism: Such a definition is clearly a prerequisite in order to answer the main question that the paper addresses. The answer to this question will be presented in Section 5, in which the requirements of the Bayesian model, specially pragmatic, will be spelled out, and the misunderstandings of Bayesian model in experimental studies exposed. Finally, in Section 6, several questions related to the scope of Bayesian model will be considered and some of its limitations will be pointed out.

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[1] One can think of four major exceptions: Cohen's (1960) pioneering work on "psychological probability"; Hammond's theory of social judgment (Hammond 1955); Anderson's theory of the integration of information (Anderson 1991) and research on "fast and frugal heuristics" (Gigerenzer et al. 1999). More generally, and from a descriptive point of view, it has often been claimed that normative models in general, and the Bayesian model in particular, require unrealistic computational faculties, and are therefore poorly adapted to the realistic description of human judgment (Chase et al. 1998, Oaksford and Chater 1992). Bayes' identity (see Appendix 1), in particular, demands the combination (multiplication, addition and subtraction) of at least three different numerical items.

2. Descriptive and normative uses of the Bayesian model in experimental studies of probability judgment

The review of studies of probability judgment (whose references will be detailed in this and the next Section) points to the general conclusion that human beings do not seem to perform probability judgment in a "Bayesian" manner. Experimenters can have two interpretations of this negative result, depending on whether they are concerned with the coherence of human probability judgment or with the descriptive adequacy of the model.

First, the Bayesian model is maintained as a model of reference, and the experimenter makes use of it to appraise the coherence of human probability judgment. In line with this view, many studies argue that human judgments are not Bayesian because participants' predictions deviate significantly from the results of the experimenters' calculations based on the theory of probability and Bayes' rule (see Appendix 1) (for a review see Piatelli-Palmerini 1994; and for a recent discussion of the arguments see Baratgin 2002a). Since, in this interpretation, the normative character of the model is not questioned, many authors have naturally been led to investigate the means to remedy participants' performance.

This first interpretation was reflected in the 1960's in that part of the literature in which humans were considered to be "conservative". That is to say, human performance was not found to coincide with the results obtained by applying "Bayes' rule", which the experimenter posited as the norm of revision**.** Thus, participants' answers turned out to be less optimal than the answer calculated by the experimenter using Bayes' rule (for reviews, see Edwards 1968, and Slovic and Lichtenstein 1971).

A decade later, the well known "heuristics and bias school" used Bayesian model to underline the deviations from the predictions made by this model. These deviations were considered to result from heuristics put to use by participants. For example, some studies underlined participants' strong tendency to neglect base rates (that is, the numerical reference describing the distribution of a characteristic in a population) which, in experiments, were identified with participants' prior degrees of belief (for a survey see Koehler 1996).

Studies of this type, as well as the various controversies that they have stirred up (for example: Anderson 1991, Christensen-Szalanski 1984, Edwards 1983, Gigerenzer 1996, Hogarth 1981, Lopes 1991, Phillips 1970, Smedslund 1990), have helped reveal a number of phenomena linked to probability judgment, such as the neglect of pertinent information, and the means of reducing this neglect (for instance, by acting on the context of the task, or by presenting probabilities in a frequentist format). They have also provided many theoretical propositions about the use of heuristics and the concept of "bias" (Koehler 1996) or about the importance of the frequentist format (Gigerenzer and Hoffrage 1995).

The second interpretation of the status of the Bayesian model as a reference in experimental studies consists in considering its descriptive quality. From this point of view, it is not the coherence of the judgment that is questioned, but the model itself. According to many researchers, the discrepancies found between participants' responses and the prescriptive data calculated with Bayes' rule should not be considered as biases, but rather as a proof of the descriptive inadequacy of the normative model (Edwards 1983, Einhorn and Hogarth 1981, Hogarth 1981, Jungermann 1983, Lopes 1991). This has incited a number of investigators to give up Bayes' rule. One minimal step away consists of building alternative algebraic rules and comparing them with Bayes' identity (Anderson and Sunder 1995, Duh and Sunder 1986, Gigerenzer and Hoffrage 1995, Leon and Anderson 1974, Lopes 1985, McKenzie 1994a, Shanteau 1970, Troutman and Shanteau 1977). Going one step further, a few investigators have designed new descriptive models such as the gap model (Smith et al. 1993) and Tversky's support theory (Tversky and Koehler 1994).

Whatever the point of view adopted, there have been many criticisms of the use of the Bayesian model as a normative reference. They stem from the problem of the justification of the model (Moldoveanu and Langer 2002): Given that (i) within standard probability theory there are other rules of revision; (ii) still within standard probability theory there is another possible interpretation of probability, namely the frequentist interpretation; and (iii) beyond standard probability theory there are a number of alternative ways to model degrees of belief, why use Bayesian theory?

(i) Other revision rules than Bayes' rule may be considered within standard probability theory (Rosenkrantz 1992, Walliser and Zwirn 2002). Two of them are of special interest. One, the Dodson-Jeffrey rule (Dodson 1961, Jeffrey 1965; see Appendix 2) has already been experimentally studied and appears to present rather good descriptive qualities (Funaro 1975, Gettys 1973, Gettys, Kelly and Peterson 1973, Gettys, Michel, Steiger, Kelly and Peterson 1973, Snapper and Fryback 1971). The other is Lewis' rule (Gärdenfors 1988, Lewis 1976; see Appendix 3). To our knowledge, there is no empirical psychological study of probability revision that considers Lewis' rule (but see below).

(ii) According to some researchers, other models can explain the experimental results. For example, De Zeeuw and Wagenaar (1974) explained the phenomenon of conservatism based on a Newman-Pearson decision maker behavior and Birbaum (1983) showed that signal theory can predict answers analogous to those given by participants who neglect base rates**.** In many publications, Gigerenzer suggests using the frequentist theory of probabilities as a reference, as in his view, probability judgment improves when the experimental paradigm is presented with natural frequencies (as compared with single-event probabilities) (for a review, see Hoffrage et al. 2002).

(iii) In addition, there are a number of other possible ways to model degrees of belief, beyond the classic field of probability theory. This is the case for different models of uncertainty, which are particular in that they do not respect the axiom of additivity of probabilities, If $H \cap G = \varnothing$, then $P(H \cup G) = P(H) + P(G)$ and its corollary, the complementarity constraint, $P(H) + P(\neg H) = 1$. The following list of examples is non-exhaustive: "Probations" of Bernoulli (1713); Baconian probability of Cohen (1977); possibility theory of Dubois and Prade (1988a); evidentiary value theory of Gärdenfors, Hansson and Sahlin (1983); evidential probability system of Kyburg (1961); potential surprise theory of Shackle (1949); Demspster-Shafer belief function theory of Shafer (1976), lower and upper probability theory of Walley (1991) and non-axiomatic reasoning system of Wang (1994). These theories use different revision rules than Bayes' rule (for a survey of several forms of revision, with a practical illustrative example, see Smets 1991). On the one hand, these theories supply possible explanations for participants' responses in different experimental paradigms that are used (Cohen 1977, 1979, 1981 and 1982, Fisk and Pidgeon 1998, Freeling and Sahlin 1983, Gärdenfors 1983, Kyburg 1981 and 1983, Levi 1985, Wang 1996). These theories can also be used as norms of reference in experimental studies, as suggested by some authors (Dubois and Prade 1988b, Shafer and Tversky 1985) and as illustrated by a number of studies (Curley and Golden 1994, Da Silva Neves and Raufaste 2001, Raufaste and Da Silva Neves 1998, Raufaste et al. 2003, Robinson and Hastie 1985, van Wallendael 1989, van Wallendael and Hastie 1990, Sahlin 1983).

In the light of the foregoing points, one may wonder why it is almost exclusively the Bayesian model that has been used as a normative reference for the experimental study of probability judgment. To answer this question, we need to analyze the status of the Bayesian model in the psychological literature, that is, to identify (i) what justifications are given for its use, and (ii) what definition of it is offered.

3. Status of Bayesianism in the experimental literature

3.1. A problem of justification

First of all, there is a striking lack of justification for the use of "Bayesianism" in the experimental literature that takes it as a theoretical model of reference. This is all the more surprising as, generally, mathematical tools in and of themselves are rarely the objects of studies in psychology. Nevertheless, one can find a few explanations, either historical or conceptual, for the use of the Bayesian model in psychology.

Historically, its use came in the wake of experiments on models of decision theory. As early as the 1950's, the link had already been established between economic decision theory and psychology (Edwards 1954). First, the studies related to the expected utility theory of von Neumann and Morgenstein (1944) reflected psychologists' interest in considering models worked out by economists in order to describe and assess agents' behavior. Second, probabilistic judgment had already been taken into consideration in the large number of studies carried out on subjective expected utility theory (for a survey, see von Winterfeldt and Edwards 1986).

Conceptually, the use of the Bayesian model is mainly a result of the attraction of certain theorists of probability towards psychology, as shown by different exchanges between specialists of probabilityand psychologists in the

Journal de Psychologie Normale et Pathologique (Cohen and Hansel 1957, de Finetti 1955, Fréchet 1954 and 1955; see also: Cohen 1960, de Finetti 1957, 1963, 1990, 1974a and 1974b, Fréchet 1951, Savage 1954). The incorporation of the Bayesian model into the experimental study of human judgment was defended by theorists of probability for diverse reasons, whereas psychologists continued to support research characterized by the absence of a normative model (Cohen 1960, Cohen and Hansel 1957). Fréchet's goal was an experimental proof that the Bayesian model is not descriptive (Fréchet 1951, 1954 and 1955). As for de Finetti, degrees of belief are the very foundation of his Bayesian theory: For this author, psychology allows one to study individuals' coherence, and even to correct their possible errors (de Finetti 1957, 1963 and 1990). The attraction that the empirical domain exerts on theorists was even played out in certain empirical work, which could be directly tied to psychology (see Fréchet 1954, de Finetti 1963 and 1965). Similarly, the first study to explicitly use the Bayesian model as a reference was directed by a theorists of probability (Rouanet 1961). The article of reference, which opened the Bayesian model field of research towards the study of probabilistic judgment, was co-signed by probability theorists and a psychologist (Edwards et al. 1963). Finally, some psychologists advocate the use of the Bayesian model as a norm of reference because of its intrinsic "descriptive qualities" applicable to human judgment (Ajzen and Fishbein 1975, McCauley and Stitt 1978, Peterson and Beach 1967).

3.2. A problem of definition

The absence of a precise definition of Bayesianism is the source of a second surprise. In experiments that use it as a normative model of reference, predictions derived from it are compared to participants' answers. If there is a divergence, the studies conclude either that participants are irrational or that the Bayesian model presents poor descriptive properties, without having clearly defined, from the start, what Bayesianism is. Like for any model, Bayesianism has its own constraints and implications, which must be identified before any experimental work. We review the three main – often implicit – uses of the word "Bayesianism" that can be found.

(i) In psychological literature, there appears to be an implicit convention according to which the term "Bayesianism" is almost always equated with the simple use of Bayes' identity (Ajzen and Fishbein 1975 and 1978, Bar-Hillel 1980, Beyth-Marom and Arkes 1983, Birbaum 1983, Chase et al. 1998, Fischhoff and Lichtenstein 1978, Gigerenzer and Hoffrage 1995, Girotto 1994, Griffin and Tversky 1992, Kahneman and Tversky 1972b and 1973, Lewis and Keren 1999, Lopes 1985, Manktelow 1999, McCauley and Stitt 1978, Mellers and McGraw 1999, McKenzie 1994a, Slovic and Lichtenstein 1971, Sedlmeier and Gigerenzer

2001, Stanovich and West 1999, Wolfe 1995). In these articles, the terms "Bayesian" or "Bayesianism" just refer to the use of Bayes' identity.[2]

This convention is visible in the main paradigms of the literature on base rate neglect where the experimenter supplies the base rate information in the instructions. This information is deemed by the experimenter to be the only possible prior probability. The experimenter also supplies a message (quantitative or qualitative), representing the evidence, which allows a calculation of likelihood. The experimenter then calculates the posterior probability with Bayes' identity. Participants' responses are then compared to this reference result. Several tasks applying this paradigm have become famous; they are the "urn and chip" problem, in the spotlight during the 1960's (Rouanet 1961), the "medical diagnosis" problem (Hammerton 1973), the "cab problem" (Kahneman and Tversky 1972a), and the "engineer-lawyer problem" (Kahneman and Tversky 1973).

(ii) Another conventional use of the term "Bayesianism" occurs sometimes as a synonym of probability theory, without offering an interpretation of the concept of probability. A violation of the axioms of probability theory by participants is then characterized as a non-Bayesian behavior. One such example concerns the "conjunction fallacy" (Tversky and Kahneman 1982). Commentators of these observations often conclude to the non-Bayesian thought of participants (Piatelli-Palmerini 1994), which is an over-specification, given that the results go against probability theory at large (and only, as a particular case, against Bayesianism).

(iii) Finally, occasionally the term "Bayesianism" is associated with a subjective interpretation of probability (Edwards 1968, Fischhoff and Beyth-Marom 1983, Tversky 1974, Wallsten and Budescu 1983).[3] However, specifications concerning the constraints and, above all, the consequences of this kind of interpretation for experimental work are left unspecified. For instance, in one of the most famous and most often quoted papers in the literature on probability judgment, Tversky and Kahneman (1974) explicitly take the viewpoint of subjective probability. They acknowledge that, from this viewpoint, "any set of internally consistent probability judgments is as good as any other" (p. 1130). It is nevertheless in this paper that they have summarized the results of their research program on heuristics and biases obtained by using a standard methodology which is appropriate for studies of the calibration type but, as will be shown in a later section, is incompatible with the subjective interpretation of probability if it aims to study coherence. There are, however, a few exceptions that deserve to be highlighted, such as John Cohen (1960) and Phillips (1970). Cohen (1960) took the care to analyze the main interpretations of probability, and specially the subjective conception which can be found in the writings of Keynes and

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[2] For instance, the famous paper by Gigerenzer and Hoffrage (1995) which underscores the importance of the frequentist format in using Bayes' identity is entitled, "How to improve Bayesian reasoning without instruction: Frequency formats".

[3] Of course, the authoritativeness of many authors mentioned in the present paper is acknowledged. Our point concerns the *use* that they had of the term "Bayesianism" in their writings.

de Finetti. Phillips (1970) also underlines the subjective character of probability in Bayesian theory and criticizes the methodology used in the “urn and chip” problem which is based on the objective interpretation of probability.

In conclusion, with very few exceptions, most investigators of probability judgment (i) either have conventionally identified the term “Bayesianism” with the use of Bayes' rule, or (ii) more seriously, have left it undefined, with the consequence that the adoption, as a norm of reference, of this undefined model, can only be left unjustified. But if the use of a model is to be justified, it must be defined rigorously beforehand. In particular, it is essential to specify the interpretation of probability which underlies Bayesianism, a question to which we now turn.

4. Status of Bayesianism in probability theory

As a matter of fact, there are different interpretations of probability. Good (1950), for example, lists nine interpretations of it. For the time being, we will limit ourselves to the distinction between the *objective* and the *epistemic* conceptions of probability. The objective interpretation refers to the notion of a random, empirical, and physical phenomenon. In this case, probability is understood as a determined and unique object which exists outside the individual. Notice that according to this interpretation, the concept of the probability of a single case is hard to conceive of because the repetition of events is necessary to calculate the probability, which is identified to a frequency. On the other hand, the epistemic interpretation consists in viewing probability as the result of an individual's judgment which depends on his/her knowledge. This judgment is identified with the individual's degree of belief. As will be seen later, the experimental work cannot remain unaffected by the interpretation that is chosen, in terms of the paradigms, of the materials, as well as of the interpretation of the results.

Defining Bayesianism in a unique manner seems extremely difficult. Not without irony, Good (1971) attributes eleven facets to Bayesianism, and he underscores the ambiguity of this word on several occasions (Good 1975 and 1976). This stems partly from the lack of independence of the various interpretations of probability (Good 1971, Kaye 1988) and also from a confusion between Bayesianism and theories which do no more than using Bayes' identity or one of its consequences. Using de Finetti's own words, this amounts to assimilating the “Bayesian standpoint” with the “Bayesian techniques”. Now the latter, which are the mathematical corpus of probability theory related to the various ways of stating Bayes' rule (Appendix 1), have been the object of a revival of interest in the past decade, especially in the Artificial Intelligence community. In this community, as in mathematics and in the “hard” sciences at large, it has become common practice to use the term “Bayesianism” in the second sense, which is perfectly justified. But as the term in its first sense (the “Bayesian standpoint”) has philosophical and psychological implications, these should be made explicit when it appears in the context of psychological investigations. The nature of the "Bayesian standpoint" should be made explicit before debating about its use.

In his famous 1763 essay, Thomas Bayes defined the essence of what was subsequently called "Bayes' identity".[4] In the same work, he recommended resorting to a uniform prior probability distribution in a situation of total ignorance (subsequently called the "principle of indifference"). It has been suggested that these two principles could constitute the foundation of what is commonly called "Bayesianism" (Gillies 1987). However, they do not allow a satisfactory characterization, whether separately or jointly. One, the acceptance of Bayes's rule underdetermines the adherence to Bayesianism, whatever it may be, because the use of this rule constitutes the basisof the "Bayesian techniques" which are endorsed by anyone who understands elementary algebraic calculation; in brief, this criterion cannot be a discriminating criterion to define Bayesianism: *The various formulations of Bayes' rule are permitted in all different possible interpretations of probability* (Allais 1983). Two, the principle of indifference is not sufficient of its own, or even in conjunction with the adoption of Bayes' rule because, while it makes an hypothesis regarding probability distributions, it fails to provide a definition of the concept of probability.

A solution to this definitional problem can be found in the writings of a few authors who have proposed a global definition of the Bayesian standpoint focused essentially on the internal coherence of an individual's judgment (Gärdenfors 1988, Good 1971 and 1976, Skyrms 1975; and, in a more elaborate form, Seidenfeld 1979). In line with this approach, a general definition of Bayesianism will now be proposed, based on three criteria: One general epistemic criterion and two criteria defining a double notion of coherence (Baratgin 2002a); see also for a similar presentation Seidenfeld 1979).

(a) Epistemic criterion

An individual's degrees of belief are interpreted in terms of probability. Any person is supposed to be able to assign a probability of realization to any event, including single occurrences. This obtains even in a situation of uncertainty or total ignorance. The conception of probability theory as a way of modeling degrees of belief indicates that probability is completely subjective. Following this epistemic criterion, probability represents the measure of the degree of belief depending on the individual's (general) state of knowledge K, at a given moment t. This implies that all probabilities are conditional. The degree of belief in H should be understood as the conditional probability $P(H|K)$. For the sake of simplicity, let it actually be $P(H)$.

[4] More precisely, what appeared in Bayes' essay is the definition of conditional probability (see the first formula in Appendix 1). It is Laplace, in his "Mémoire sur la probabilité des causes par les événements", who offered, in 1774, the first formulation of Bayes' identity (the second formula in Appendix 1) (see Laplace 1986).

The next two criteria define a double notion of coherence that degrees of belief must respect. One is based on a static hypothesis and the other on a dynamic hypothesis (Hacking 1967).

(b) Static coherence criterion

Degrees of belief must obey additive probability theory axioms (Kolmogorov's axioms, see Appendix 4).

(c) Dynamic coherence criterion

Revision of an individual's degree of belief given a new evidence D is derived from Bayes' rule. This revision can be conducted in two different manners.

First, one can imagine D as certain, and estimate the posterior probability P(H|D) at a time t_0, although at time t_0 one has no knowledge whether D will occur or not. When one estimates P(H|D), one must in fact pretend that D actually occurred at a time t_1 (with $t_0 < t_1$).

Second, one may revise ones' degree of belief upon learning the realization of D. One would then revise ones' degree of belief P(H) at time t_1 (with $t_0 < t_1$) when one gets informed that D occurred at time t_1. Let this probability be $P_D(H)$).

The dynamic coherence criterion also called the "*(Bayesian) conditioning principle*" (Hacking 1975, Seidenfeld 1979) posits that the revised probability upon learning the outcome D at time t_1 is equal to the probability of H conditioned on the (imagined or assumed) evidence D at a moment t_0 (that is, P(H|D) yielded by Bayes' rule):

(vi) $P_D(H) = P(H \mid D)$.

In fact there is no distinction between a probability assessed conditionally with regard to a message D, and a probability given the realization of this message D.

Two sub-sets of the various probability theories satisfy the three coherence criteria (in particular the epistemic criterion). They coincide with a traditional distinction between two sorts of Bayesianism, namely "logical Bayesianism" and "subjective Bayesianism" (de Finetti 1972, Earman 1995, Good 1950 and 1975, Hacking 1975, Kyburg 1961, Seidenfeld 1979, Walley 1991). The difference coincides in turn with the opposition made by Kyburg (1988) between epistemic and doxastic uncertainty.

• Logical Bayesianism

For logical Bayesians (Cox 1946, Good 1950, Jaynes 1994, Jeffreys 1931, Keynes – the founder – 1921) subjective probability is epistemic in the sense that it results from an evaluation based on the body of evidence that is available and does not depend on the agents' state of mind. More precisely, for logical Bayesians, two individuals with the same state of knowledge will emit the same judgment of probability. In other words, there is a unique distribution of probability for a given state of knowledge. With regard to the dynamic process of probability revision, two individuals with the same state of knowledge are supposed to make the same revision when they receive the same message. Logical Bayesians justify the status of Kolmogorov's axioms by an appeal to desiderata that specify logical coherence (Cox 1946, Good 1950, Jaynes 1994).

• Subjective Bayesianism
For subjectivists, probability depends on the general state of mind of the individual expressing a belief. Its doxastic character implies that no unique probability exists even if one considers that the body of evidence is given. This allows two agents with the same state of knowledge to formulate two different evaluations of probability, as long as these evaluations are coherent (in the static and dynamic senses). However, in certain situations, one may hope that the evaluations of many individuals converge towards the same judgment. For example, most people may think that flipping a fair coin presents a probability of 1/2 for landing heads-up. But, in many situations, even people with the same knowledge will give diverging judgments. In other words, there is no unique distribution of probability for a given state of knowledge. Taking an agent's state of mind into account also reflects on the dynamic process of revision of degrees of belief. There can indeed be a difference in the way two individuals acquaint themselves with one given message. Two individuals may revise their degrees of belief differently, even if they receive the same information, because each one will go about the revision in keeping with their own "state of mind". The notion that acquiring a piece of knowledge includes subjectivism was developed by Ramsey (1926), and independently with a few nuances by de Finetti (1930, 1993, 1964 and 1990). The necessity for an agent to obey Kolmogorov's axioms (the static coherence criterion) is defined by the "Dutch book" argument:[5] The agent's degree of belief in an event can be revealed by the odds at which he is willing to bet on this event**.** The simple argument of coherence, namely, avoiding the possibility that an opponent wins with certainty whatever the outcome, leads to Kolmogorov's axioms and to the definition of conditional probability (de Finetti 1930 and 1964, Ramsey 1926; see also, for a discussion of this argument, Skyrms 1975).

In the light of either one of these two interpretations, Bayesianism appears to be a clear candidate for a model defining a behavioral norm of human probabilistic judgment. The two fundamental varieties are invested with the same normative aspect. The theory should serve as a guide for human probability judgment (see de Finetti 1965 and 1976, Jaynes 1994). However, only the subjectivists sometimes present the normative theory as a possible candidate for a *descriptive* theory of reality:

> The rules of the calculus of probability, conceived as conditions necessary to insure coherence among the assignments of probability of a given

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[5] Other justifications for using Kolmogorov's axioms as coherence constraints have been put forward by subjective Bayesians, such as the "scoring rules" argument (de Finetti 1963) or Savage's (1954) model of subjective expected utility.

> individual, constitute, in fact, only the precise expressions of the rules of the logic of the probable which are applied in an unconscious manner qualitatively if not numerically, by all men in all circumstances of life. (de Finetti 1964, p. 111).

According to supporters of subjectivism, the experimental study of probability judgment is indispensable to identify people's possible errors and subsequently improve their spontaneous judgments (de Finetti 1961, 1963 and 1976). It seems natural for psychologists whose main objective is to study people's degrees of belief to consider a theoretical model that characterizes the individual's idealized doxastic states. In psychology, some authoritative researchers have explicitly referred to "subjective Bayesianism" while choosing a model of reference (Edwards et al. 1963, Edwards and Phillips 1964, Kahneman et al. 1982, Tversky 1974). This is why it is this subjective interpretation of Bayesianism that will be considered in what follows.

5. Constraints of Bayesianism on experimental studies

We now possess a normative model of reference for probability judgment clearly defined by the three criteria listed earlier: An agent is coherent in the Bayesian sense just adopted if the agent's degrees of belief (which are conditioned on his/her general set of beliefs) respect the axioms of Kolmogorov (static coherence) and the agent revises them by Bayes' rule (dynamic coherence). We are in a position to answer the question that motivates the present study, as we can identify the constraints on the experimental methodology resulting from the criteria within the subjective interpretation of probability. We will examine the extent to which these constraints have been satisfied in psychological studies, while considering whether they can be satisfied in principle.

5.1. Applying a pragmatic approach to the experimental study of probability judgment and revision

In psychological experiments on thinking, participants are typically presented with the statement of a problem that refers to general knowledge or that is integrated in an original specific scenario, following which either they are provided with a result and asked to decide whether or not it follows from the statement (the evaluation paradigm) or they are asked to calculate or evaluate the value of a variable (the problem solving paradigm). From the experimenter's point of view, the derivation of the response is justified by the application of principles specific to the domain under study (e. g., physics, economics, probability, etc.) These principles belong to a theoretical model (either normative or descriptive) which it is the aim of the experiment to assess by examining participants' performance. In the experiments on judgment, the question is often a request for a comparison or for a qualitative or a quantitative evaluation, etc. The experiment may be administered orally during an interview with the experimenter, or in a written form, using paper and pencil or a computer. Whether the presence of the experimenter is physically real or mediated by the support of a written message, there are two interlocutors actually engaged in a communication, so that it is appropriate to apply a conversational analysis of this communication. In particular, laws of language use in the spirit of Grice's (1989) maxims of conversation are applicable.

After they have been provided with the instructions and the problem statement, the participants are presented with the target question. Like any utterance, this question must be interpreted. Generally, its meaning is not straightforwardly identifiable because the information may be more or less long, conceptually hard, vague or ambiguous. As for any question, its interpretation is determined by the content of the putative answer: The answer should satisfy the expectation of relevance attributed by the participant to the experimenter. The term "relevance" is being used here in its theoretical sense (Sperber and Wilson 1995): An utterance is relevant if it has cognitive effects for the hearer; these consist in altering degrees of belief or in drawing contextual conclusions, that is, conclusions that follow from the message together with previous information.

Now, in experimental settings (as well as in instructional settings, and more generally in testing situations) the participants are aware that the question put to them is a higher order question, that is, the question implicates "the experimenter wants to know whether I know how to find the answer". The interpretation of the question is determined in part, and revealed, by the specific kind of knowledge that the participant chooses to exhibit through his/her response: The participant bases this choice on the assumption that what is relevant to the experimenter is to know whether the participant has that kind of knowledge. This choice and the underlying assumption reveal in turn the participant's *representation of the task*. This is why knowledge of the population tested is essential. The experimenter must anticipate the range of questions of interest that participants are likely to attribute to him/her, in the light of their educational and cultural backgrounds. This should contribute to the determination of the verbal and non verbal materials to use in order to avoid possible misinterpretations of the task or, when using standard paradigms, this requires a macroanalysis of the information provided. Until the late 1980's, these issues were neglected by investigators of thinking and reasoning, even though social psychologists had related interests in the past that were linked to concerns about the transparency of the experiment (see the concept of *demand characteristics* proposed by Orne 1962).

There is, in addition, another kind of analysis, based on linguistic pragmatics, which needs to be applied to the sentences used to state the problem. The output of this analysis is the determination of the interpretation of the statement and question that the participant is likely to work out; it delivers the actual proposition(s) that will be processed during the inferential and judgmental treatment, taking into account the frame of the task representation. The reason to perform this microanalysis is that it is an essential step to guarantee the validity of the experimental task. Indeed the experimenter is interested in the processing of specific information which he/she expects the participant to recover from the sentences used in the problem statement. Unluckily, the interpretation of the message reached by the participant often differs sharply from the interpretation intended by the experimenter. The latter typically (although not automatically) wishes to communicate the literal meaning of the problem statement, whereas the former often adds some meaning. It is an essential part of pragmatic theory to explain by which process this implied meaning, called an *implicature,* is generated by the hearer (here, the participant). It is clear that a judgment can be deemed to have been made in accordance with some principle only to the extent that the interpretation of the problem statement and of the question made by the participant and the experimenter coincide. This approach to the experimental study of thinking and reasoning, often called "conversational approach", has been suggested or applied by a number of researchers (Adler 1991, Hilton 1995, Macchi 2000, Politzer 1986 and 1991, Politzer and Macchi 2000, Schwarz 1996). It is quite general and can change our understanding of some tasks radically (see Sperber et al. 1995 for Wason's selection task). In the field of probability judgment, it sheds new light on a few classic tasks used to study the base rate fallacy (Krosnick et al. 1990, Macchi 1994 and 1995, Schwarz et al. 1991, Politzer and Macchi 2005) or the conjunction fallacy (Dulany and Hilton 1991, Politzer and Noveck 1991). Now, there is a supplementary reason to plead for the use of language pragmatics in the case of probability judgment when the chosen model is Bayesian. Pragmatic analysis is mandatory in order to identify the participant's interpretation of the experimenter's message; in the specific case of revision, this concern applied to the new information (the evidence) is even more crucial. The linguistic theory explains how an individual reaches the interpretation of a message (what is communicated) which often differs sharply from the linguistic meaning of the message (what is expressed). This interpretation has a propositional form to which a degree of belief is attached. This interpretation process is essentially individual and there is no reason why its product should be identical from one person to another (Shafer 1985). In brief, the inferential process of belief formation that results from communication is one of the main concerns of pragmatics; since Bayesianism posits that the agent emits probabilities (i.e., degrees of belief) as a function of what (s)he knows and learns, it is clear that the experimental study of Bayesian probability judgment and revision must rely on pragmatic theory. We consider in turn the constraints imposed by the three criteria in the light of pragmatics.

5.2. Constraints due to the epistemic criterion

Within the subjective interpretation of Bayesianism, a consequence of the epistemic criterion is that in experimental situations, the subjective character of participants' probabilities must be taken into account. For Bayesians, "probability does not exist" (de Finetti 1990 and 1976), in the sense that probability corresponds to a degree of belief with the same ontological status as any belief or opinion. This interpretation is at variance with the frequentist interpretation, for which probability exists as a mathematical or an empirical object.[6] This difference is even more marked for the notion of prior probability in Bayes' identity (de Finetti 1951). This is because the evaluation of the initial probability P(H) is closely linked to the interpretation of probability that is adopted. For Bayesians, this *prior* probability is always a conditional probability. This is so in Bayes' identity: $P(H \mid D) = \dfrac{P(D \mid H)P(H)}{P(D)}$ in which P(H) and P(D) are to be understood as probabilities relative to our current knowledge (de Finetti 1951 and 1972). They are conditional probabilities with respect to the individuals' initial knowledge K and set of beliefs B (P(H) = P(H|K, B)).[7] From a Bayesian point of view, it is impossible to estimate participants' probability without knowing precisely their set of beliefs together with the conditions on which they express their probability.

Thus, because probability has no objective value, a probability judgment is never correct or incorrect. This implies that *we can in no way judge the quality of an answer taken in isolation from the individual's set of beliefs*. De Finetti (1970) warned that the question should specify the information with regard to which individuals are asked to condition their probability. From a modern, pragmatically based viewpoint, these conditions are often epistemic implicatures that may, or may not be drawn, depending on the representation of the task. He also made it clear that the questions should refer to specified events rather than to classes of events, for which an additional assumption of uniformity may, or may not be made, by the individual. Even more drastically, he claimed that "an event is a specified assertion rather than a specified fact" (p. 131). It follows that even if two assertions are co-referential, the "events" referred to may not be given the same probability. So, besides the fact that semantic variation affects the degree of belief in an event, when such a variation is possible, pragmatic effects may be induced, based on the existence of a contrastive choice: Using one expression rather than another available co-referential expression may generate an epistemic implicature which affects the degree of belief.

In summary, one may well be coherent from a Bayesian point of view, and yet give an evaluation that is very far-off from an expert's evaluation because the individual's initial set of beliefs may not reflect an objective state of the world or the expert's evaluation of it. Is there a means to access these degrees of belief? Before considering this question, which concerns individuals, we consider the question of the statistical treatment of the results obtained from a group of individuals.

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[6] However, Bayesian theory does not automatically reject frequentist information in order to work out (subjective) probability estimate. It may be the case that a coherent subjective judgment of an event probability equals the frequency of occurrence of this event (de Finetti 1964).

[7] According to the frequentist interpretation, probability is defined as the limit of a frequency. It is unique and in no case does it depend on the individuals' knowledge (the epistemic criterion is not respected). In this view, the phrase "the probability of event E is P" indicates that probability of event E is a property of the event itself which possesses an objective value that can be calculated or approximated by means of logical or physical operations.

It has been a long time since Newell (1981) criticized the common method used in the study of thinking, which presupposes that all participants solve problems in the same way, a methodological error which he called “the fixed-method fallacy”. More recently, the notion that there are individual strategies in reasoning has been more and more accepted (Schaeken et al. 2000). As far as the subjectivist Bayesian model is concerned, the situation is even more radical: *No two participants can a priori be assumed to be equivalent*. In effect, if it is assumed that each participant is characterized by his/her own initial set of beliefs, and arrives at a probability estimate (the result communicated to the experimenter) that is dependent on it, the former as well as the latter may differ from one participant to the other. In addition, personality factors may influence participants’ degrees of belief. For example, for a same state of knowledge, there is a tendency among anxious participants to overestimate the probability of a serious negative event, as compared to non anxious participants (Beck 1976). The consequence is inescapable: In order to study individual coherence, averaging data across participants is meaningless, and only individual-based differential measures are adequate. This questions the adequacy of the studies devoted to probability judgment in the past decades, as most of them have used statistics based on averaged data. Of course, the observation that, say, eighty percent of a population commit what looks like the violation of a law of probability points to a phenomenon that requires an explanation; but the phenomenon does not necessarily reveal a lack of coherence in the Bayesian sense. This may be the case, but in order to demonstrate it, the methodological requirements that we are reviewing in this and the next Sections must be satisfied.

We now consider the question of the participants’ degrees of belief in experimental settings. There are two related problems. One is the problem of how to specify for the participants an uncertain situation without introducing uncontrolled factors susceptible of altering their degrees of belief; the other is the problem of the measurement of these degrees of belief.

It is a reasonable assumption that the representation of the task and the confidence in the statements which frame an experimental situation are in a relation of mutual adjustment. On one hand, the nature of these statements (e.g., overtly fictitious or explicitly pertaining to scientific knowledge, in particular mathematical, etc.) contribute to the representation of the task (e. g., participants think they should draw on their imagination, or on their knowledge and mathematical skills). On the other hand, the representation of the task contributes in turn to define subjective probability: In one type of task, it may be appropriate to assume that a propositional content is certain whereas in another one the same assumption might be inappropriate. In brief, the choice of the materials, content and questions should not only be determined by the theoretical concept that the experiment aims to investigate and by the population (as in any study of thinking); in addition, this choice should be made with a view to fixing, or at least controlling, the participant’s degree of belief in the critical propositional content of the problem statements.

To what extent has the epistemic character of probability been taken into account in past studies? Most of the time, experimenters consciously or not have adopted an objective interpretation of probability. Presenting a probability numerically reflects the assumption that it exists independently of the experimenter. What the studies actually achieve is a comparison between the participants' answer and the "true probability" calculated by the experimenter. From a Bayesian point of view, independently of whether or not these studies show correct calibration of participants' probabilities – the majority, in fact, show bad calibration – this methodology is not focused on individual coherence, but rather on a rationale of analysis of accuracy, where the experimenter plays the role of the expert who formulates "correct predictions". Therefore, arguments which are supposed to demonstrate the non-Bayesian character of human probability revision (like exploiting the phenomena of conservatism, of base rate neglect, or the effect of the frequentist format) do not appear to infirm the Bayesian hypothesis of coherence. The epistemic criterion applies to all probabilities: Prior probabilities, likelihoods and posterior probabilities. In particular, base rates, used as prior probabilities by the experimenter, do not necessarily constitute the prior probabilities considered by the participants, as de Finetti and Savage warned:

> From a subjective point of view, prior probabilities are arbitrary in the sense that logic or experimental knowledge are not sufficient to impose or exclude the choice of any specific distribution. But in fact, they are not totally arbitrary, for they make sense only insofar as any of these distributions has been chosen by an individual in order to represent his opinion (de Finetti and Savage 1962, pp. 83-84, our translation).

Furthermore, as we have seen, they are not necessarily equal across individuals. Thus as many authors have already remarked, an agent's failure to use base rates as prior probabilities is not a sign of non-Bayesian behavior (Cosmides and Tooby 1996, Gigerenzer and Murray 1987, Kahneman and Tversky 1983, Koehler 1996, Levi 1981 and 1983, Logue 1995, Niiniluoto 1981, McCauley 1996, McKenzie 1994b, Phillips 1970, Skyrms 1981). But, more fundamentally, one might question the very possibility of knowing the prior probability used by each participant. As far as the target estimate is concerned, it should ideally be expressed spontaneously, because the mere request for such an expression changes the individual's initial set of beliefs, so that the belief that is actually measured is conditioned on a new set of beliefs, namely the initial state modified by the request (Borel 1939). It seems doubtful that this requirement can be satisfied while using the usual tasks in which scenarios are presented.

Since the sixties, various techniques of assessment have been proposed in order to measure subjective prior probabilities. The main result of these studies indicates that different techniques may prompt different responses (see for example Seaver et al. 1978, Winkler 1967). Yates (1990) distinguishes two kinds of assessment techniques, which recalls *statement methods* and *inference methods* (see also Chesley 1975). In the statement methods "the opinion is conveyed by a likelihood statement, an explicit pronouncement of a person's degree of certainty that an event will happen" (Yates 1990, p. 16). In the inference method, the degree of belief is inferred from the behaviour of individuals who are engaged in activities such as betting or lotteries. Now, a drawback of the majority of statement methods is that experimenters are prevented from applying these techniques to non experts as these methods are based on metacognitive knowledge of probability distributions and, more generally, of probability theory (see Winkler 1967). Only two statement methods appear suitable at first sight: The magnitude estimation, which consists of asking what is the probability that X is less than or equal to Y, and the ratio estimation. Unfortunately, although the former seems easy to understand by naive participants, it requires the intervention of a reference for which on can suspect within-subject variance. In addition, the latter seems to rely on participants' familiarity with odds, which seriously limits the possibility to draw general conclusions: Indeed, as noted by Wright (1988) "people do not adequately understand odds as a response mode without extensive training".

Among the inference methods are the use of betting and lotteries. In both cases, the individual is asked to make a choice between two or more alternatives, and the experimenter infers the probability from the expressed preferences or indifferences. Unfortunately, these methods actually reveal both utility and probability in the form of expected utility and cannot be considered as appropriate to measure pure subjective probability.

In conclusion, the appropriate methods to measure subjective probabilities are still to be discovered in order to be confidently applied to a population not familiar with betting and probability theory. This conclusion holds even though on several occasions de Finetti proposed an operational definition of probability that reveals belief by means of an induced behaviour. This is the argument of "scoring rules" (de Finetti 1962, 1963, 1965, 1970 and 1990). In phase 1, participants attach a number x to each event E. In phase 2, on the outcome, participants receive a penalty of x^2 if the event E becomes true and a penalty of $(1-x)^2$ if E does not turn out to be true. In the assessor's view, the value of x needs to be the one that is the closer to the participant's degree of beliefs in order to minimise the expected loss (see de Finetti 1990 for a proof). This method is often used in experiments based on forecasts of real events (sport results, marketing, management accounting) in which experimenters strive to assess the accuracy of participants' responses. However, this method does not seem directly suitable for tasks requiring forecasts of fictional events as can be found in experiments of psychology (because it is necessary that one should know whether or not the target event has occurred). As a consequence, serious questions on the validity of these studies can be raised, as this method is the only one acknowledged by subjectivist Bayesians.

In summary, the epistemic criterion has deep psychological implications; whether related assumptions are plausible, and in the affirmative, whether the constraints which they impose can be satisfied in the laboratory, are fundamental questions. The validity of the experimental investigation of probability judgment, past and future, depends on the answers to these questions. We have presented reasons to doubt that that the answers can be affirmative.

Hopefully, new paradigms may be discovered, which will enable investigators to overcome the difficulties linked to the representation of the task.

However, even if the experimenter controls the message actually received by the participant, the former has no access to the state of belief of the latter. Then, the following question arises: How to investigate an individual's revision whose initial set of beliefs is not well known and may not have even remained stable by the time the new message reaches him or her ? It is not clear whether one can find an answer. In other words, even if one could eliminate the source of uncertainty that is of a pragmatic nature, there would still remain a fundamental uncertainty based on the experimenter's incomplete knowledge of the participant's set of beliefs.

5.3. Constraints due to the static coherence criterion

Let us assume nevertheless that the conditions of the interaction with the experimenter and the experimental materials are such that the participants can express their actual degrees of belief. Let us assume also that we have an operational definition of a degree of belief through an appropriate behavior. We now envisage the consequences of Kolmogorov's axioms on the experimental methodology.

The first axiom will not be discussed, as it can hardly be regarded as a constraint. Rather, it is a convention by which numerical values are attributed to the lower and the upper bound of the scale of measurement of degrees of belief.

The second axiom posits that the measure of a probability maps events onto real numbers strictly comprised between 0 and 1. Now, the psychological status of this scale should be carefully considered: On one hand, the scale can be used for purpose of communication, and it is no more than a useful conventional tool. It is not illegitimate, but it requires at least some justification which is seldom found in the psychological literature. De Finetti (1962) compares it with a temperature scale: By training and experience, people become able to translate their degrees of belief into numbers as they do for their perception of temperature. On the other hand, it may be tempting to give the]0, 1[scale the status of a format of mental representation for degrees of belief. But, of course, this has little psychological plausibility, and it is incompatible with subjective Bayesianism. From this viewpoint, probabilities are essentially qualitative degrees of belief which are not mentally represented by numbers, let alone real numbers (except, once again, at a metacognitive level as a result of training or formal instruction). It is true that in some situations, people can express probabilities by fractions, but these situations constitute a special case where they deal with countable sets of possibilities.

The common practice that consists of using a numeric scale when asking participants to express their probability, as well as when discussing results by comparing participants' responses against a calculated value that belongs to the scale, introduces an illusion of accuracy. In addition, this might cue participants towards representing the task as a mathematical exercise and solving it accordingly, whereas using non numeric presentation might be more neutral. Leaving aside metacognitive uses of probability, people usually express their degrees of belief in natural language using non numeric expressions: Expressions of frequency (never, seldom, often …) or of certainty (doubtful, likely, sure …) that are susceptible of being ranked on various scales. There is a large body of research on uncertainty expressions; a few have concerned themselves with the relative merits of verbal and numeric scales (Budescu et al. 1988, Erev and Cohen 1990, González-Vallejo et al. 1994, Rapoport et al. 1990, Windschitl and Wells 1996, Zimmer 1983). Among the investigations that aim to determine the mapping between numeric and verbal scales, there is a frequent implicit assumption to the effect that the numeric scale would represent the "correct" scale. Moxey and Sanford (2000) remark that the question addressed has been "how accurate are verbal expressions", rather than "what different patterns of thought and reasoning might be set up by numbers and verbal expressions". Comparing a verbal scale with a numeric scale belongs to a calibration methodology. It is true that verbal expressions are more natural, as they are not based on a mathematical convention. But the important point is that from subjective Bayesian point of view, both kinds of scale are but a way of communicating degrees of belief, and that the only interesting quality of this communication tool is its capacity to help observe changes in degree of belief susceptible of revealing internal coherence or lack thereof. From this viewpoint, the literature in question does not seem conclusive.

The study of the additivity axiom, and of its corollary, the constraint of complementarity, implies, from the Bayesian point of view, the use of a within-participant methodology together with the control of the pragmatic factors. To our knowledge, this has never been applied (see Baratgin 2002a for a review). In case an apparent violation would be observed *within-participant* (for example, suppose that $P_{participant}(A \cup B) > P_{participant}(A) + P_{participant}(B)$), the experimenter should verify that $A \cap B$ is assumed by the participants to be empty. Several within-participants studies show a possible violation of the complementarity constraint: Robinson and Hastie 1985, van Wallendael and Hastie 1990, Villejoubert and Mandel 2002, Windschitl 2000). However, in none of these studies did the experimenters verify whether their definition of the set of the possible alternative hypotheses coincided with that of the participants. Furthermore, the experimenter should be in a position to make sure that the probability estimated by the participant coincides with the target probability. It is not clear whether this is possible in principle.[8]

A similar, and even greater difficulty arises with the fourth axiom (which introduces the concept of conditional probability, Appendix 4) as the experimenter should ascertain *within-participant* that: $P(H \cap G) = P(H)P(D \mid H)$.

[8] The Bayesian model relies on a very constraining criterion of coherence due to the axiom of additivity of probabilities. As we have underlined, other possible ways to model degrees of belief exist, based on a weaker coherence criterion obtained by loosening the axiom of additivity. For each of these normative models, theorists have proposed specific sets of axioms that degrees of belief should satisfy. From a technical point of view, some of these models, and notably all of the probability interval models, can be considered as generalizations of the Bayesian model (and especially of its subjective variety, see Walley 1991) with less constraining criteria, rather than alternatives to it. Taking these models as norms of reference would relax the constraint of additivity, but would not solve the methodological difficulties already mentioned.

In summary, Kolmogorov's axioms impose constraining hypotheses on the nature and the processing of degrees of belief. Before assessing people's performance in probability judgment by way of the Bayesian model, one ought to consider the psychological status of these hypotheses in terms of their plausibility. Either the plausibility of the hypotheses is debatable, or it is not clear how they can be verified, or both.

5.4. Constraints due to the dynamic coherence criterion

The first step in order to study the criterion of dynamic coherence is to check whether the agents respect the criterion of *static* coherence. In effect, a violation of Kolmogorov's axioms might have consequences on the revision process. For example, there are cases where the violation of the complementarity constraint is accompanied with conservatism (Marks and Clarkson 1972; Phillips, Hay and Edwards 1966) or with base rate neglect (Baratgin and Noveck 2000, Davidson and Hirtle 1990).

Assuming the static coherence criterion to be satisfied, to study the dynamic coherence of answers is to examine the agents' respect of the "conditioning principle". The probability revised in relation to imagined evidence must be equal to the probability revised in relation to the same evidence once it is realized.

As a consequence, *before any request for a revised probability estimate, the prior estimate should be independently measured*. For instance, in the cab problem, participants' degree of belief in a cab being blue should be obtained (using an appropriate method of measurement) prior to the witness' information. However, this method could create problems. It might be the case that, when learning the new message, the participants in fact use a prior probability that differs from the prior probability expressed before learning the new message.

Another consequence is that it is essential that *the experimenter control the source and conditions of delivery of the new message*. The reason is that the degree of belief in the new information depends on the reliability of the source. For instance, an individual who needs to retrieve information from long-term memory may feel more or less confident in the truth of that information and give it more or less high subjective probability.

On the methodological side, clearly, the classic paradigms are not dynamic examples of probability revision. In almost all of the studies, both the base rate information (supposed to be the prior probability) and an informative message are provided. The dynamic process of revision cannot be studied in this purely static context. One would at least need to know the prior probability for each participant, in order to know how (s)he modifies it once (s)he is aware of a new message.[9]

[9] Results of the few studies that follow this methodology indicate that participants produce revised probabilities close to those calculated by the experimenter using Bayes' identity and participants' prior probabilities and likelihoods as input (for example: Baratgin 2002b, Evans et al. 1985, McCauley and Stitt 1978, Peterson et al. 1965).

The respect of the "conditioning principle" has never been considered in classic studies on conservatism and base rate neglect, and has been studied very little in a direct fashion (see, however, Waller and Mitchell 1991). Some results in the existing literature can be understood as violations of this criterion. This is the case, for example, with the result observed by Gigerenzer, Hell and Blank (1988, experiment 1) for the engineer-lawyer problem compared to Kahneman and Tversky's study in 1973. In this experiment, participants personally draw at random the personality descriptions, whereas in the initial paradigm, participants are informed of the random drawing by a statement. In the initial situation, participants must "imagine" the random drawing (the evidence D), while in Gigerenzer et al.'s (1988) situation, they learn of the evidence D directly. In this case, there is a better use of base rates in Kahneman and Tversky's condition. Thus, participants seem to have degrees of belief different in case they learn evidence D than in case they imagine D (see also Baratgin and Noveck 2000). For logical Bayesians, this result may indicate a violation of the principle of Bayesian conditioning. From a subjective viewpoint, it is hardly adequate to speak of a violation because pragmatically the evidence D is not the same in the two situations. In Kahneman and Tversky's (1973) situation, the information of the random drawing (D) is not "understood" as certain by the participant, who learns in fact a reported observation (D*). If this is the case, the result can be explained by the difference in conditional probabilities between the two experiments ($P(H|D^*) \neq P(H|D)$ if $D^* \neq D$)). In addition, in order to consider the possibility that a violation of the Bayesian principle of conditioning has occurred, the result should be obtained in a within-participant design.

One may question the feasibility of experimentally testing the conditioning principle, $P_D(H) = P(H|D)$ for essentially pragmatic reasons. In order to perform such a test, an individual should be instructed to imagine or assume that D occurred and asked to give an estimate of P(H) (i.e., $P_D(H)$); then, the same individual should be told or given evidence that D actually occurred and finally asked an estimate of P(H) (i.e., $P(H|D)$). But such a sequence of questions which have great apparent similarity can only look anomalous to the participant. Ideally, the participants should express their degrees of belief spontaneously, or they should do so at least on one of the two occasions.

In summary, the implications of the kind of subjectivism which is at the heart of the concept of probability in the subjective variety of Bayesianism, and the other kind of subjectivism implied by the same Bayesian point of view, which stems from the notion that any probability judgment is a conditional probability that depends on the individuals' belief, make it doubtful that experimenters can be faithful to the subjective Bayesian concept of probability, for measurement reasons and for communication reasons. The use of an unspecified model has resulted in a practice of "calibration" of participants' judgment in which the experimenter plays the role of the expert setting the result, a practice which should be given up as the aim of this methodology, namely a conclusion about individuals' rationality, is unattainable. Also, the importance of the dynamic aspect of probability revision has not been taken into account. Because the necessity for the experimenter to understand how the new message is received and interpreted by the participants engaged in the process of revising their degrees of belief has not been recognized, there is a lack of authentic research on the dynamic coherence embodied in the conditioning principle.

6. Relativising Bayesianism

Independently of the difficulties encountered in measuring degrees of belief, there is yet another point to consider, namely the domain of application of the Bayesian model and its possible limitations for the study of probability revision. We consider in turn two limits of Bayesianism for the experimental study of probability revision; they concern the background of revision and the problem of the null prior.

6.1. The background of revision

As any model, the Bayesian model applies to a specific range of situations. As far as probability revision is concerned, it limits the background of revision, in the sense that the hypotheses given in the initial statement always remain unchanged. This means that the "world" of reference is invariant, so that the situation is that of a static world. This case of revision, called *revising*, reflects the entire set of existing experimental paradigms. In these experiments, the participant cannot doubt the initial statement's hypotheses. However, another case of belief revision exists, one where the new message may announce a change in the initial hypotheses. In other words, the older world (the initial hypotheses) is modified, which creates a dynamic world situation. This is the *updating* case (Katsuno and Mendelzon 1992).[10] Let us consider, for example, a situation where a researcher submits a paper and is awaiting the referees' answers. The author's chances of being accepted can be estimated by imagining various evidences: We are in a *revising* background. Let us now suppose that in that lapse of time, a new article appears on the same topic, which casts doubt on the chances of the first paper. We are now in an *updating* situation. In an *updating* situation, Bayes' rule is not the appropriate rule of revision. Walliser and Zwirn (2002) show that in this case the adequate rule of revision is Lewis' rule. Revision is carried out by imaging: An agent considers a distribution of probability on possible hypotheses (possible worlds). Following a message invalidating a possible world, the degrees of belief concerning this world are redistributed over the other possible worlds that the agent considers as the closest to the invalidated world (Appendix 3). Pearl (2000) offers a similar analysis in distinguishing between the observation of evidence D and the observation of a deliberative action causing D (in this later case, Pearl uses the operator *do(D)*. In the first situation, the world remains unchanged and Bayesian conditioning by the passive observation of D is justified. In the second

[10] In distinguishing between *revising* and *updating* (which are two different kinds of revision), we follow a well-established distinction in Artificial Intelligence.

situation, the action *do(D)* modifies the world and it is Lewis' rule that captures this transformation).

Therefore, theorists should carefully consider the domain of application of the Bayesian model and refrain from a systematic use of Bayes' rule as a reference. Moreover, the experimenter should check that the participants' representation of the task coincides with the type of revision situation intended by the experimenter.

From an experimental point of view, to our knowledge, the study of Sloman and Lagnado (2005) is the only one that has taken into account exactly the particular background of *updating*.[11]

6.2. A technical limitation: The case of the null prior

As Bayes' identity formula shows (see Appendix 1), P(H|D) is a linear function of P(H). In particular the "absorbing" property of zero applies. If an agent has a null degree of belief in a hypothesis (P(H) = 0), with Bayes' identity the degree of belief in this hypothesis will always remain null, regardless of the new message. One may think that this constraint is not adapted to the modification of degrees of belief as observed in real life. Indeed, van Wallendael and Hastie (1990) and Curley and Golden (1994) have observed the phenomenon of "hypothesis resurrection", meaning that hypotheses previously estimated at zero by participants later have a probability different from zero. Nevertheless, these studies did not concern themselves with the way people learn new messages. Pragmatically, this phenomenon raises the problem of requesting quantitative responses from a participant when individuals' degrees of belief are close to zero. This limitation in the use of Bayes' rule makes it necessary for psychologists to consider the revision rules that are compatible with the "hypothesis resurrection" such as Lewis' rule (see Walliser and Zwirn 2002 for a review of the other compatible rules).

Lewis' rule is appropriate as a formal answer to the two limitations of Bayesianism for the study of probability revision. However, the same operational difficulties remain as those encountered for the study of the conditioning principle in relation with the subjective character of the degrees of belief and the message learnt; they are compounded by another difficulty due to the subjective character of the notion of a distance between two possible worlds inherent in Lewis' rule (Appendix 3).

[11] There is a domain in which the notion of possible worlds seems applicable, and therefore the use of Lewis' rule justifiable, namely counterfactual reasoning: Theorists of this domain explicitly refer to the philosophical discussion of possible worlds (Roese and Olson 1995). More precisely, the analysis of counterfactuals leads one to consider, on one hand, the ease with which the antecedent condition can be altered (see Kahneman and Miller's 1986) construct of mutability, of which one factor is clearly linked to plausibility or feasibility, and ultimately to the proximity to the actual world); and on the other hand the closeness of a possible expected outcome to the actual outcome. It would seem that individuals have an intuitive notion of a "distance between possible worlds". This notion is the basis of the process of revising by Lewis' rule (see Appendix 3).

One possibility to study probability revision in the two types of situation (revising and updating) independently of Bayes' and Lewis' rules would be to compare the fundamental qualitative properties by which they differ. These properties have, in fact, the status of rationality postulates (see Walliser and Zwirn 2002 for a representation of these postulates[12]). However, it is again difficult to make this solution operational. Take for example two of the properties derivable from the postulates which differentiate revising from updating, namely *commutativity* and *idempotence*. The commutativity property of revision simply indicates that learning message A followed by message B has the same effect as learning message B followed by message A, if A and B are independent of each other. The idempotence property stipulates that the arrival of a new message that is already known should not modify the degree of belief initially assigned to the same message. These two properties are fulfilled by Bayes' rule and by Lewis' rule. Intuitively, the respect of these properties in the revision process seems to be a reasonable assumption. However, one may have different expectations based on pragmatic theory. For example, as far as commutativity is concerned, relevance theory (Sperber and Wilson 1995) would predict an order effect when different messages appear in turn. Generally, a message has greater contextual effects when it is learnt in the last position, and this is all the more marked as it is more strongly opposed to the first message (see also Krosnick et al. 1990) for an analysis of order effect by pragmatic considerations inspired by Grice's theory).

Regarding idempotence, pragmatic theory predicts also that upon reception of a new message that is identical to the preceding one, the agent will search for a reason for this redundancy and will interpret it, for example, as an indication of its importance or reliability. These apparent violations of properties of revision rules cannot be considered as fallacies in the subjective setting of learning of evidence. Indeed the pragmatic view simply means that the experimenter's evidence D is not the same as the one the participant uses. There is a possibility that a strict subjectivist interpretation of Bayesianism renders the rules unfalsifiable because any departure from these is susceptible of a pragmatic explanation, given the standard experimental methodology in which the participant perceives that the message is directly or indirectly under the experimenter's control. In other terms, studying whether there really is a violation of properties implies building experimental paradigms that are immune from the interpretive factors linked to the representation of the task. In order to help satisfy this requirement, researchers should design new paradigms (e. g., leading the participants to believe that they are finding the evidence themselves in order to proceed with their revisions) or shift from the laboratory to carefully planned field observation.

In summary, the focusing of the literature regarding probability revision uniquely on the Bayesian model has resulted in the neglect of other normative models. One, the notion of *coherence* is multiple and should not be defined exclusively within the framework of additive probabilities, on which the Bayesian

[12] For example, one of the important postulates is *Strong conservation*. It is common to the systems of axioms for probabilistic revising and updating. It states that if a message is already validated by the initial belief, the final belief is unchanged: if $P(D) = 1$, then $P_D(H) = P(H)$.

model is dependent; and two, the Bayesian model confines the experimental study of probability revision to a situation where the new message does not alter participants' set of beliefs (a static world background). Future research should take into account the various existing models, even though many of them are generalizations of the Bayesian model. The research on the coherence of probability revision should be redirected to the revision process in which the agent's set of beliefs is modified (a situation of dynamic world). Lewis' rule is appropriate in such cases but experimentally it is hard to operationalize. Instead, one might investigate the rational foundations of Bayes' rule (for the revising situation) and Lewis' rule (for updating situations) but the direct experimental study of these postulates amounts to testing predictions derived from pragmatic considerations.

7. Conclusion

For almost a half-century now, experimental research on probability judgment has been based on the use of a so-called "Bayesian model". In spite of this, this stream of research has failed to carry out a thorough analysis of the conceptual foundations of the model that could have led to uncover its methodological constraints and its psychological implications. This has had two broad damaging consequences.

The first consequence is a failure to distinguish between Bayesian *techniques* (i.e., just applying Bayes' rule as a predictive revision rule) and Bayesian *point of view* which implies the adoption of the epistemic character of probabilities and the fundamental notion of coherence engendered by the model. In this model, Bayes' rule is not just a rule of combination of quantitative probabilities, but it results from both the static coherence axioms (Kolmogorov's axioms) and the criterion of dynamic coherence postulating the principle of conditioning. This misinterpretation of Bayesianism in the experimental literature has resulted in a failure to recognize the constraints and the implications of the model for the experimental studies. In the subjective interpretation of Bayesianism, the experimenter must consider, by definition, any probability judgment as the simple expression of a degree of belief. In this framework, a probability judgment can in no case be regarded by the experimenter as true or false; it can only be regarded as coherent or incoherent, depending on whether it respects the probability axioms and the conditioning principle.

The second and more fundamental consequence results from the first. Once the conceptual content of the model has been unveiled, it appears that the principles which define subjective Bayesianism and the consequences that can be derived from them are not experimentally testable: This is because there is no satisfactory operation by which subjective degrees of belief can be measured. Even if probability could be measured, insuperable methodological difficulties would remain. These are mainly due to the conditional nature of probability and the impossibility to access all the relevant conditions on which it depends; in particular, the operation of measurement is in itself a factor susceptible of altering the probability to be measured. The study of the qualitative properties of degrees of belief, which at first sight could be thought of as an alternative to the study of the coherence of degrees of belief is also problematic in the subjective interpretation of Bayesianism. It is hard for the experimenter to come to a conclusion about possible violations of the properties of the model (like the rationality postulates and other properties such as commutativity and idempotence) if they can be explained by pragmatic mechanisms.

Are these reasons sufficient to reject subjective Bayesianism as a normative model of human probability judgment and revision? Probably not, as we have not demonstrated its inadequacy. The foregoing analyses indicate that this is no more possible than to demonstrate its adequacy. Those who wish to maintain the model may be motivated by the admirable ease and elegance of its derivation from a simple postulate of rationality (essentially the "Dutch Book" argument, see Section 4) which gives it a kind of Platonistic perfection. Thus, they have to rely on the principles of parsimony and simplicity to justify their adherence to the model, and this is not unreasonable. But for those, among whom the present authors rank themselves, who are more attracted to empirical criteria of evaluation such as operationalization than they are to formal virtues, the verdict is that the mind might well be Bayesian but we will never know whether this is the case or not.

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Appendix 1

One can put into question the extensively used term of “Bayes’ rule”. The literature indeed offers several specifications of “Bayes’ rule”. Although these specifications are equivalent, it appears important to distinguish three major forms. Given two alternative hypotheses H and $\neg$H, one event D such that D $\neq \varnothing$, let P(H) be the prior probability of H, P(H|D) the posterior probability, P(D|H) and P(D|$\neg$H) the likelihoods and finally P(D) the probability of the data D.

Form 1, “conditional probability”: $P(H \mid D) = \dfrac{P(H \cap D)}{P(D)}$,

Form 2, “Bayes’ identity”: $P(H \mid D) = \dfrac{P(H)P(D \mid H)}{P(D)}$,

Form 3, “Derived Bayes’ identity”:

$$O(H \mid D) = \frac{P(H \mid D)}{P(\neg H \mid D)} = \frac{P(H)P(D \mid H)}{P(\neg H)P(D \mid \neg H)} = O(H)\frac{P(D \mid H)}{P(D \mid \neg H)} = O(H)L(D \mid H).$$

The vast majority of studies have exclusively used the second form of Bayes’ rule (b). Few studies have used the third form (c), considering more specifically the phenomenon of conservatism (for example Phillips and Edwards 1966) or the base rate neglect (for example Kahneman and Tversky 1973). Pieces of research quoting the first form of Bayes’ rule (form a) are much less numerous (see nevertheless Gigerenzer and Hoffrage 1995, Lewis and Keren 1999, Mellers and McGraw 1999).

Appendix 2

In the 1960's, several studies in various fields (computer science, psychology and medicine) strove to define a rule that would hold in the case where the new evidence D is not certain (let it be D*). Dodson (1961) addressed this problem. An uncertain message D* can lead one to elaborate subjective judgment on the probability of D being true, thus yielding $P(D|D^*)$ and its complementary $P(\neg D|D^*)$. His idea was to incorporate $P(D|D^*)$ and $P(\neg D|D^*)$ in the conditioning of H based on the notion of expectation. Dodson suggested that posterior probability must be equal to the sum of the probabilities conditionally to D and $\neg D$ multiplied by the probabilities of being in the situation where D is true or where $\neg D$ is true. Dodson (1961) established the following rule: $P(H|D^*) = P(D|D^*)P(H|D) + P(\neg D|D^*)P(H|\neg D)$.

One can notice that in the degenerated cases where $P(D|D^*) = 1$ and $P(\neg D|D^*) = 0$, the rule reduces to simple conditioning $P(H|D^*) = P(H|D)$ and $P(H|D^*) = P(H|\neg D)$ when $P(D|D^*) = 0$ and $P(\neg D|D^*) = 1$, respectively. In other words, Dodson's rule can be viewed as the generalisation of Bayes' identity. Independently, Jeffrey (1965) proposed the same rule of revision. However, in the subjective interpretation of the Bayesian model as specified in the present paper, an individual can revise his/her probability directly when learning D*, and it can be shown that, conversely, Bayes' identity is equivalent to the Dodson-Jeffrey rule (under the assumption that D* depends on D but not on H, see for example Gettys and Wilke 1969, Jaynes 1994, Schaefer and Borcherding 1973, Schum and du Charme 1971).

Appendix 3

Gärdenfors (1988) showed that Lewis' (1976) rule (*imaging*) can be analysed as a rule for probabilistic revision. In this case, the methodology of revision is different from the one provided by Bayes' rule. We simply intend here to give an intuitive idea of it (for a detailed technical analysis, see Gärdenfors 1992, Pearl 2000, Walliser and Zwirn 2002). In order to explain this rule, it seems convenient to use Kripke's (1962) possible worlds semantics. An agent considers a distribution of probability on possible worlds. Following a message invalidating a possible world, degrees of belief concerning this world are redistributed on the other possible worlds that the agent considers to be the closest to the invalidated world. Let us consider the following example (Dubois and Prade 1994, Walliser and Zwirn 2002). A basket may be described by four worlds depending on whether it contains an apple (a) or not (¬a) together with a banana (b) or not (¬b). Someone believes at t_0 that the basket contains at least one fruit: $K = (a \wedge b) \vee (a \wedge \neg b) \vee (\neg a \wedge b)$. Let us assume that the person's prior probabilities associated to each possible world are 1/2 for $(a \wedge b)$, 1/3 for $(\neg a \wedge b)$, 1/6 for $(a \wedge \neg b)$ and 0 for $(\neg a \wedge \neg b)$. A *revising* message brought up by a reliable direct witness informs the person that there is no banana: $A = (a \wedge \neg b) \vee (\neg a \wedge \neg b)$. The revised belief K*A is now $(a \wedge \neg b)$, so that the posterior probability allocated to $(a \wedge \neg b)$ is 1. This is what Bayes' identity gives:

$$\left(\begin{array}{l} P_A(a \wedge \neg b) = P((a \wedge \neg b) \mid A) \\ P_A(a \wedge \neg b) = \dfrac{P(a \wedge \neg b)P(A \mid (a \wedge \neg b))}{P(a \wedge \neg b)P(A \mid (a \wedge \neg b)) + P(a \wedge b)P(A \mid (a \wedge b)) + P(\neg a \wedge b)P(A \mid (\neg a \wedge b))} \\ P_A(a \wedge \neg b) = \dfrac{1/3 \times 1}{1/3 \times 1 + 1/2 \times 0 + 1/6 \times 0} = 1 \end{array}\right)$$

But with Lewis's rule (or imaging) one will end up with a different result. An *updating* message says at t_1 that there is no more banana. In this updating situation, the revised belief K*A is $(a \wedge \neg b) \vee (\neg a \wedge \neg b)$ because the reasoner starts afresh a distribution over all possible worlds. In order to do so, the prior probabilities (1/2 and 1/3) attached to $(a \wedge b)$ and $(\neg a \wedge b)$ are distributed over other close worlds. One can think that the "world" $(a \wedge b)$ is closer to $(a \wedge \neg b)$ than to $(\neg a \wedge \neg b)$ (choosing arbitrarily the intuitive physical action as a basis to define the distance) and therefore the degree of belief 1/2 attached to $(a \wedge b)$ is attributed to $(a \wedge \neg b)$: $\left(P_A(a \wedge \neg b) = 1/6 + 1/2 = 2/3\right)$.

Alternatively, one can think that the " world" $(\neg a \wedge b)$ is closer to $(\neg a \wedge \neg b)$ than to $(a \wedge \neg b)$ and therefore the degree of belief 1/3 attached to $(\neg a \wedge b)$ is attributed to $(\neg a \wedge \neg b)$:

$$\left(P_A(\neg a \wedge \neg b) = 1/3\right).$$

This example illustrates the two situations of revision using, of course, a distance arbitrarily chosen in the *updating* case. In the latter case (Lewis' rule), the initial belief can be thoroughly modified, since one could expect to have no fruit anymore in the basket following the message.

Appendix 4

Kolmogorv's axioms contain the following two "convexity rules" (the impossible event is noted Ø and the set of possible results Ω):

(i) $P(\emptyset) = 0$ and $P(\Omega) = 1$;
(ii) H is said to be "significant" if and only if $0 < P(H) < 1$;

and the additivity rule:

(iii) If $H \cap G = \emptyset$, then $P(H \cup G) = P(H) + P(G)$

together with its corollary, the complementarity constraint $P(H) + P(\neg H) = 1$.

The conditional probabilities must satisfy the rule of compound probabilities:

(iv) $P(H \cap G) = P(H)P(G \mid H)$;

and its corollary, namely Bayes' identity (Appendix 1).